John Marshall Esq
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THE PRACTICE OF ARTIFICIAL ANÆSTHESIA, LOCAL AND GENERAL,

WITH ESPECIAL REFERENCE TO THE MODES OF PRODUCTION, AND THEIR PHYSIOLOGICAL SIGNIFICANCE.

Introduction to a Discussion in the Section of Pharmacology and Therapeutics, at the Annual Meeting of the British Medical Association in Cardiff.

BY DUDLEY W. BUXTON, M.D., B.S., M.R.C.P.,
Administrator of Anæsthetics at University College Hospital.

MR. PRESIDENT AND GENTLEMEN.—In attempting to carry out in the best way the honourable and onerous task imposed upon me, that, namely, of opening a discussion upon the practice and theory of anæsthesia, I think I shall best subserve the end in view by briefly sketching the plan upon which my subject will be treated, before plunging into such details and by-issues as are incident to so wide-reaching a theme as that we have assembled to discuss.

I may remind this Section, that twenty years ago, artificial anæsthesia was discussed under the ægis of our great Association, but in the Section devoted to physiology. Then pharmacology and therapeutics were not what they are now, and hence the relegation of the purely practical subject of anæsthesia to physiology. But now, let us remember that in the Section in which we sit we have to know the physiological side of our therapeutic agents, and to apply this knowledge to the practice of grappling with disease and assuaging pain. And can we carry out such a scheme for anæsthetics, can we, with hope of profit or success, seek to disentangle the thread and unfold a rational explanation of the physiology of anæsthesia, to be followed by the wholly practical considerations involved in the skilled and beneficial application of anæsthesia for surgical or medical practice? It seems to me, that such a course is the only one likely to advance the knowledge of our subject, and to prevent undue prominence from being bestowed upon mere details of practice, details important enough in themselves, but surely subservient to the consideration of those great laws of life and death which underlie our practice in the induction of anæsthesia.

We may start with a proposition that the end and aim of anæsthesia, however applied, is comprised in the rendering a given area insensitive to pain. The more perfect the agent in use, the more truly localised must be its action; and conversely, the more widely distributed be its action, the less safe is the anæsthetic in question. Now, to illustrate this proposition, let us briefly sum up the physiological bases of life. We may, perhaps, be permitted to regard them as a duality, a conscious voluntary portion wherein we meet with receptive end-organs whose function must be to convey impressions for our purpose, we will say painful impressions, from the periphery by nervous strands to the central receptive and perceptive centres, the cerebro-spinal axis; and the second member of this duality is the nervous mechanism whereby the wholly involuntary and unconscious functions of life are carried out. To these we must add the blood-stream, and its physiological behaviour towards the cells, whereby their due regeneration, growth, and function, are performed.

Now clearly anæsthesia might be attained by cutting off the afferent channels from peripheral nerve-endings to nerve-centres, thus leaving these last unaffected. This was proposed long ago by Dr. Waller, who suggested the compression of nerve-trunks, to obviate the pain experienced when their peripheral branches were divided. And again, anæsthesia must result if the sensory paths of the spinal cord, or the perceptive centres of the brain, be acted upon by some agent which annuls their function. In the first case, we have the subject of local anæsthesia brought before us, the abrogation of painful sensation over a given area, and determined by a locally applied agent; in the second, we find ourselves face to face with the wider and, I venture to think in the present state of our knowledge, more important subject of general anæsthesia.

I have yet to learn if any agent, applied locally, fulfils the requirements of the ideal anæsthetic; whether it wholly removes pain when peripheral structures are divided, and yet leaves healthy tissues uninjured, while the nervous structures of the cerebro-spinal axis remain unaffected.

Among agents which have been proposed to effect general anæsthesia, I would have us ask ourselves which does most nearly approach the ideal anæsthetic—namely, the one which, robbing the patient of that intensity of sensation which we call pain, will yet not overflow, so to speak, the nervous system, and by its action interfere with rational life; secondly, and most important of all, which will not impede the due carrying ou of organic existence.

In some carefully executed researches commenced in 1882, Dr. Brown-Séquard sought to show that analgesia without sleep could be induced by irritation of the mucous membrane of the larynx, of the trachea, and of certain skin-areas. He found (*Comptes Rendus*, June, 1885) that, by using certain (irritating) vapours, he could, in the case of monkeys, dogs, and various other animals, induce a condition in which these creatures gave no evidence of painful sensation, even during cutting or other painful procedures. His method was to allow carbonic acid gas, or chloroform-vapour, to play upon the larynx or trachea, after having done a preliminary tracheotomy to prevent the access into the lungs of the irritating vapour. Stimulation of the trunk of the vagus above the origin of the superior laryngeal nerve, and even irritation of the skin of the back, will, he asserts, induce this sleepless analgesia. His view is, that the irritation of the superior laryngeal nerves exerts a reflex inhibitory action upon the brain-centres—an explanation which appears to me almost too vague for us to accept, save as a working hypothesis. In applying this extraordinary discovery to the practical issue of inducing surgical anæsthesia, we are met by formidable difficulties. Brown-Séquard had an arrangement whereby his patient breathed pure air for from two-thirds to three-fourths of a second; he then breathed carbonic acid gas, and immediately emptied his lungs. Thus he would take less than half the ordinary period of inspiration for pure air. Brown-Séquard's discovery cannot be said to have yet entered into the domain of practical anæsthetics; but he assures me that the subject will ere long be placed more fully before the profession.

I may here mention an experience of Dr. Péan's, which, I fear, we seldom meet with, and this is that, in a certain number of cases, he, Péan, had found that ether will, while rendering the patient analgesic, yet permit him to retain his consciousness. Indeed, we are told that the patients watched the steps of the operation with evident interest.

I have belief that such cases occur sufficiently frequently to deserve a more thorough inquiry being made into their nature. They are, at least, most curious, and have a truly important bearing upon the question now before us.

As it will clearly be impossible for me to attempt an examination of each member of the large group of carbon compounds which have been used as anæsthetics, I will, with your permission, restrict my attention to some of the most important members of the group. This method, I am convinced, will prove the only possible one, as the subject is so wide, and the amount of information so prodigious, that we should hardly profitably deal with more than a few agents. And I would venture to express an opinion that what we, as workers in this important field of pharmacology, most need, is more thorough and intimate knowledge of individual anæsthetics than a superficial, if showy, acquaintance with the plethora of agents which already gluts the anæsthetist's armamentarium.

The prevailing views advanced to explain anæsthesia by the carbon compounds may be summarised: (1) that they act by lessening oxidation of the tissues; (2) that they induce physical changes in the blood, which, in their turn, induce anæsthesia; (3) that they produce anæmia of the brain; (4) that they are capable of affecting nervous tissue directly.

The first view, advanced with considerable force by Dr. Snow, which again and again cropping up as the subject of anæsthesia has come under discussion, may be said to have been finally disposed of at the hands of the committee appointed by the Royal Medical and Chirurgical Society (*Medico-Chirurgical Transactions*, vol. xlviii, p. 329), who showed that chloroform narcosis, although analogous to, is not, in fact, an asphyxia. Claude Bernard (*Anesthésiques et Asphyxie*, p. 97, 1875) also points out that, whenever asphyxia occurs during chloroform narcosis, it happens only as a result of the narcosis, and is not the precedent and determining cause of that state. And, further, we find actual proofs of these statements; for Paul Bert (*Leçons*, 1870) found in experiments instituted upon dogs, that the following changes in the blood occurred. Before anæsthesia, 7.5 per cent. of oxygen existed in the blood; during the stage of resolution, 12.4 per cent.; and similar results occurred in other experiments.

The only period in which venosity of the blood appeared was when the violent struggles of the individual under experiment impeded respiration, while they enhanced tissue-metabolism. Nor is the evidence brought to a close here; for the committee appointed by our Association, which, in 1881, produced their most careful and valuable report, found that the exhalation of carbonic acid gas was actually increased during chloroform narcosis.

Dr. Richardson has also shrewdly pointed out that there are very many agents, such as nitrite of amyl, powerful stayers of oxidation, which do not assume the part of anæsthetics. Hence the statement "narcotism is suspended oxygenation" (Sansom on Chloroform, p. 62) must, I think, be taken as non-proven.

Nor, indeed, will the second theory survive the test of experiment. Ludwig Herman (quoted by Wood; *Therapeutics*, 279), found that, in the lower ranges of the animal world, among infusoria, etc., chloroform produced a species of narcosis. It has also been shown that, if all the blood be drained from frogs, and an artificial saline medium be injected, they live with apparent normality. Now, if these frogs be subjected to the influence of chloroform vapour they become narcotised. (Lewisson, Reichert's *Archiv. f. Anat. und Physiologie*, 1870.) In a similar way, we find members of the vegetable kingdom affected by the vapours alike of ether and of chloroform. The leaves of the sensitive plant cease to shrink if narcotised, and seeds will remain without any signs of germination when exposed to narcotic vapours; while aquatic plants may be rendered quiescent, as far as growth is concerned, when similarly treated. Now, in all these cases physical changes in the blood can have nothing to do with the matter. It is interesting to note that these narcotic effects in plants, like similar phenomena in animals, are purely transitory. It is an abeyance of vital existence; the thread of life will again be taken up when the narcotic agent is removed. The theory based upon the assumption that anæsthesia supervenes upon anæmia of the brain, seems to be a case of *post hoc non propter hoc.* Cerebral anæmia certainly supervenes on sleep, and on the torpor provoked by anæsthetics. In the functionally active anæmic brain of the frog, chloroform will induce narcosis. Such, and other considerations into which I have no time to enter, seem to me abundantly to show that these theories are untenable. We therefore have to fall back upon an hypothesis which formulates that the action of this group of anæsthetics is one exerted upon the nervous system itself. We shall also see that to this view a rider will have to be appended, to the effect that this action is one which falls upon the protoplasm of the nervous and of the muscular systems.

Bernstein's (Schmidt's *Jahrbücher*, Band 142) researches are also confirmatory of the view that the nerve-centres are themselves directly affected. He found that the action of chloroform upon peripheral nerves was very slight, thus rendering it very doubtful how much of the struggles and excitement of the second stage of narcosis is due to spinal irritation. Bernard and Bert certainly are opposed to the last view. The former holds that the struggles are purely physical, and shows that after section of the spinal cord no such movements occur. It must then, I think, be conceded that, although these vapours can gain access to the nervous system by the lungs, by serous membranes, and possibly by the skin, yet the action exerted is one directly upon the nervous centres themselves.

Prevost found that, if the encephalon of a frog were touched with chloroform, after deligation of the aorta, narcosis resulted, which passed off when, the ligature having been removed, the blood-stream again found its way to the brain, sweeping away the chloroform.

In numerous experiments, with which I will not detain you, it has been shown that when the centres are protected, narcosis or anæsthesia is not produced; but that, when the muscles and nerves are similarly treated, while the centres are exposed to anæsthetic vapours, anæsthesia obtains. But, as I shall point out in a moment, chloroform has various ways in which it affects the nervous system. These, for convenience, we will call the direct and the reflex. By the direct, I mean when the chloroform is conveyed by the blood-stream to the cerebro-spinal tissues, and produces gradual extinction of the activity of the centres therein found. The reflex action of chloroform is that action which I believe chloroform to be able to exert upon the peripheral terminations of the vagi, giving rise to cardiac inhibition and sudden death.

Speaking of this reflex action of chloroform, I am anxious that we clearly separate the cases of pure syncope, due to fright, to shock—as when operations are commenced before adequate anæsthesia has been attained—from those which result directly from the action of chloroform. Let us consider the last cases first. The mechanism of such deaths is, I think, clearly enough proved to be as follows.

The patient has presented to him an atmosphere impregnated with chloroform, up to a high percentage value. This atmosphere acts as a powerful irritant on the naso-pharyngeal and laryngeal areas, and produces a reflex inhibition alike of respiration and of cardiac rhythm. The patient seems to hold his breath for an instant, and then grows pallid. At this moment his pulse will have ceased, respiration will be in abeyance. In support of this view, I would bring the following facts to your notice. The Committee appointed by the Royal Medical and Chirurgical Society of London, state that, when chloroform was given to dogs, as to the human being, the breathing was often arrested as if by spasm.

Professor Rutherford (*Journal of Anatomy and Physiology*, vol. iii) has also shown that, when a rabbit is exposed to an atmosphere in which is diffused an irritant, it stops breathing; subsequently the heart ceases to beat, and the rabbit expires. Rutherford thus explains these results. He believes that the fibres of the inferior laryngeal nerves are induced to inhibit the action of the heart. In cases in which a preliminary division of the vagi had been performed, this stoppage of respiration did not occur, and of course the heart remained beating. Dr. Richardson found that quite the same phenomena showed themselves when chloroform was the irritant employed. I may also refer to the Research Committee of the British Medical Association on this point. In their fourth Report (BRITISH MEDICAL JOURNAL, December 18th, 1880), they describe a sudden fall of blood-pressure following administration of a heavily laden chloroform atmosphere, while, on withdrawing the anæsthetic, recovery took place, and they add, "This occurrence followed so uniformly upon certain stages of chloroform-narcosis, upon every approximation of the sponge containing the agent, even for a few seconds, to the animal's muzzle, that it was regarded as probably reflex."

But although these facts will, I submit, go far to explain the rationale of those melancholy cases of death when "only a whiff of chloroform" had been administered, we must bear in mind that deaths resulting, I think, from chloroform-irritation, occur at a somewhat later period; namely, within the first minute or two—certainly during the interregnum between volitional and unconscious life. Such deaths are far more common than are those of the preceding class. Dr. Brown-Séquard says, "It is by reflex influence due to the sudden irritation of the branches of the par vagum in the lungs, that chloroform has killed in the very rare cases in which the heart's action has been stopped before the respiration." Brown-Séquard proved that no sudden syncope could be induced in dogs after he had divided the vagi. He ascertained also that he could produce quite the same effect by galvanising the medulla oblongata, or the pneumogastric. In this way we see that chloroform-vapor produces death (1) by reflex inhibition of respiration, (2) by reflex inhibition of the heart; in the one case it is probable that the inferior laryngeal nerve (see Rutherford, *op. cit.*; also Rosenthal, *Automat. Nerven-Centra*, 1875), in the other, the pulmonary fibres of the vagus, are the efferent channels whereby the medulla and cardiac ganglia are acted upon.

Granting that these cases of fatal syncope owe their origin to chloroform, and I think we can hardly avoid such an admission, we discover that this agent transgresses one of the canons laid down as to the essentials of an ideal anæsthetic. In a certain number of cases, it invades areas other than sensory, and at a period when the sensory tracts are themselves still in function. Cannot we, however, lessen this evil? Is it not possible by a careful and skilled diagnosis to select such cases, and avoid giving them chloroform? Or, again, may we not so administer our chloroform as to obviate all possible risk of reflex syncope through stimulation of the vagal endings? I will not weary you with statistics, but will give you the result of careful observations and comparisons, which have led me to the conclusion that we cannot ever foretell such casualties. Those persons who seem most robust succumb, while the debilitated and feeble survive chloroform-narcosis; with, perhaps, the exception of persons possessed of a fatty heart, or those habitually liable to syncope. I think one would not be able in any manner to select cases which would *per se* be suitable or unsuitable for chloroform, or would give one either an inward assurance that the administration would be safe, or the converse.

But when we come upon the subject of the methods of administration, I think we have a more hopeful answer to record. The danger of reflex syncope arises from a sudden irruption into the lungs of concentrated vapour ; and hence, I submit, any method which employs a large volume of chloroform, or prevents thorough and great dilution, adds to the danger. The method which Mr. Clover habitually employed effectually obviated overdosage ; and so, whatever may be the drawbacks urged against it, it certainly affords a sure and valuable means of avoiding such untoward accidents. However, we have yet to seek how far chloroform confines its action to sensory or harmless areas, when the patient, having passed through the first or syncopal danger, has entered upon the true anæsthetic stage. And here I would be permitted to enter a protest against attaching the blame to chloroform in many cases when, the patient having been rendered unconscious while yet some reflexes persist, the operation is commenced. It happens only too often that "the shock" in even trifling operations—*e.g.*, circumcision, or for paraphimosis, or the extraction of a tooth, occasions death. These deaths are to be explained (Dr. Lauder Brunton, *Pharmacology and Therapeutics*, p. 174) by remembering that, the vaso-motor centre being lulled to sleep before the vagal centre is affected, the tendency to syncope is not in this case, as in the sentient being, prevented by a reflex constriction of the arterioles.

It is notorious among those who have employed this agent at all largely, that even among individuals there is a marked difference in the dose required to obtain anæsthesia. When operations on the different parts of the body are undertaken, far deeper narcosis for safety is required in some than in others ; for instance, excision of the eyeball, and operations about the genital organs. If we be content with a light narcosis in such cases, we subject our patients to grave danger.

Now, returning to the order of the advance of chloroform-narcosis : there is gradual loss of voluntary movement, the reflexes disappear one by one, all the intellectual centres are blocked, and the subject under anæsthesia is simply living, *i.e.*, has only just so much nervous action as will maintain in due working order the vital requirements of the organism. These vital requirements are readily grouped under two classes ; circulation, for which we need a healthy heart-muscle, a due correlation between it and its nervous mechanism, a certain maintenance of blood-pressure and respiration, for which is required the function of the respiratory centre, and correlation between it and the mere mechanical actions involved in respiration.

Chloroform, as was shown by Dr. Glover (*Edinburgh Medical Journal*, 1842) can affect the heart-muscle itself, although large doses are needed to obtain such a result, at all events in the case of healthy heart-muscle. Ringer (*Practitioner*, vol. xxiv), who compared the effect upon the heart-muscle of frogs when using chloroform, ether, or ethidene-bichloride, found that chloroform possessed the power of rapidly knocking down the heart, causing it at first to beat more and more feebly, until at length it was alike incapable of beating spontaneously, or yet of responding to excitation. The muscular irritability persists longer after death from ether than after fatal chloroform-narcosis. Dr. Richardson (*Medical Times and Gazette*, 1867, vol. ii, p. 481), who instituted careful experiments upon this, showed that the irritability appeared to persist longer, the smaller the equivalent of chlorine in the molecule ; and Wood (*Therapeutics*, p. 217) points out that vermicular movements persist after death from ether. Hence, anticipating a point we shall have to again broach, we may say that chloroform is certainly inimical to protoplasm at the least in a higher degree than is ether.

The action of chloroform upon circulation and respiration was shown by Snow to depend upon the strength of the atmosphere used. Clover, following up the teaching of Dr. Snow (*Proceedings of the Odontological Society*, March, 1863), shows that, if a vapour of over the strength of five per cent. be employed, it caused stoppage of the heart before cessation of respiration; when he worked with a lower percentage vapour, the heart beat for a considerable time after respiration had ceased. Mr. Clover, in making these statements, hazards the view that the human heart is probably more liable to chloroform-poisoning than is that of the lower animals.

In profound narcosis, the heart-rate slows (Committee of Royal Medical and Chirurgical Society, *Transactions*, vol. xlvii), a fact too well known to need my repeating the results of the numerous observers who have experimentally shown its truth. Dogiel (quoted by Wood, *Therapeutics*) believes that this slowing is constant, and the result of irritation of the pulmonary terminations of the vagi by the chloroform-laden blood. When preliminary section of the vagi is performed, the slowing does not, he asserts, take place. But, following upon this slowing, there is a lowering of arterial pressure (Committee of Royal Medical and Chirurgical Society, *Transactions*, vol. xiii, and Grants Committee of British Medical Association, BRITISH MEDICAL JOURNAL, 1879).

The importance of these facts cannot easily be over estimated. They reveal that the heart is, so far, working under a disadvantage, and hence will be less able to recover should danger arise from the breathing. Such conditions of the circulation are, I think, strong presumptive evidence against chloroform as an ideal anæsthetic. We are seeking simple loss of sensibility, and we find that the agent employed attacks the vital centres. The respirations, after an initial retardation, are found to become more shallow ; their rhythm may be increased, but, whether or not, there is lessening of amplitude and depth, and, at length, respiratory movements cease. It has been shown (Sir Joseph Lister, in Holmes' *System of Surgery*, third edition) that movements of the trunk, wholly ineffectual as far as respirations are concerned, often persist after respiration proper has ceased, and there is danger lest these should be mistaken by the inexperienced for true respiratory movements. Consecutive upon the cessation of respiration, we find that the pulse weakens and intermits, the heart eventually stopping. It has been found that the deoxidated blood collects in the right heart ; its power, as I have shown, being already lessened by the action of chloroform, now ceases, the muscle being unable to put forth an increased effort in response to the increased call upon it.

None will deny that such deaths occur ; about the exact etiology we cannot be so sure. Snow, Clover, and others, have taught that in percentage dosage we find the answer to our question. Given, say they, a dose less than four parts of chloroform-vapour in 100 of air, and you are safe ; increase that percentage, and danger impends. Paul Bert (*Comptes Rendus*, November, 1881), on the other hand, proposes to recognise three zones ; one, when a chloroform-atmosphere is too dilute to promote anæsthesia ; a second, in which the vapour produces anæsthesia and never kills ; and a third zone, when the chloroform-vapour percentage is always lethal. Dr. Richardson (On Death from Chloroform, *Med. Times and Gazette*, 1870) does not recognise the validity of the percentage doctrine, leaning rather to the view that the chloroform tends to accumulate in the blood, and so exerts at first a deleterious, at last a fatal effect, paralysing the sensory motor areas, at length, the sympathetic ganglia and medullary centres. In each person, it would seem to him, there exists a certain resistive power to chloroform; and in those in whom this is least, fatal issues occur, their vital centres being early invaded, and being unable to resist an influence practically inoperative in other cases.

Whether such views do more than "dimly figure out a distant truth," I cannot pretend to say ; the proofs appear unsatisfactory in some particulars. However, there seems to me no doubt at all that chloroform is a powerful protoplasm-poison, and as such its use must be fraught with danger. That chloroform is much maligned, I believe; but I venture to think that the dread of it is wholesome. Another point telling against chloroform as an ideal anæsthetic is that it depresses the bodily temperature, and so conduces to promote the effect of shock (Dumeril et Demarquay, *Archives Gén. de Méd.*, 1848).

We have yet to consider whether any other agent fulfils the canons requisite for the ideal anæsthetic.

Ether, now widely used, may be taken first. Ether, if properly administered, will produce profound anæsthesia more rapidly than chloroform. Ether has been accused (1) of killing patients by asphyxiation from spasm of the glottis; (2) of killing by provoking pulmonary mischief, bronchitis, pneumonia, etc.; (3) of paralysing the respiratory movements; (4) of inducing syncope. Syncope does certainly occur during ether-administration, although very rarely. Gosselin (*Clin. Chirurgicale de la Charité*) cites a case which occurred where an attempted reduction of a dislocated thigh was proceeding. Amidon (*New York Medical Record*), in describing the condition, believes he has succeeded in avoiding this danger by injecting small doses of atropin. One or two cases have since been recorded (BRITISH MEDICAL JOURNAL, 1878, vol. ii, p. 602). It is probably doubtful whether these cases are due to ether-inhalation, as we shall see the heart is practically uninfluenced for evil by ether. Ringer has shown how hardly ether will affect the heart-muscle of frogs; and Wood, after noting the same fact, mentions that, when it is injected into the veins of animals, the heart's action remains uninfluenced. While it is easily demonstrable that, when the mammalian heart is watched, artificial respiration being maintained, it will be found practically unaffected by the most enormous doses of ether. When death does occur from cessation of respiration, the heart beats for a considerable time. Moreover, the heart-muscle being intact, it readily resumes its functions when artificial respiration is performed. The action of ether upon the vaso-motor system has been carefully worked out. Arterial pressure is always increased in ether-narcosis.[1]

The death from spasmodic closure of the glottis is so rare as practically to be unimportant, save as a means of pointing a moral; for such cases are certainly due to the administration of too powerful a blast of ether to a timid patient. He holds his breath as long as nature permits him; and then, with the attempt at a deep-drawn inspiration, inhales a supersaturated ether-atmosphere. The delicate mucous membrane rebels, and so arises the spasm. Even in such cases, admission of air and pressure on the chest will set matters right. Such accidents, I think, rarely, if ever, occur when Clover's inhaler is employed by one accustomed to its use.

Death from chilling of the pulmonary mucous membrane, giving rise to pneumonia, or, through the direct irritation by the pungent ether-vapour, causing tracheitis and bronchitis, does in a certain number of cases occur. Sédillot (Péan, Clinique Chirurgicale à l'Hôpital St. Louis, 1882, *De l'Anæsthésie Chirurgicale*, 1882) found, experimentally, that dogs got pneumonia when ether was introduced through an opening in the trachea. Mr. Lawson Tait (*Practitioner*, March 1876), recognising this danger, invented an apparatus for preventing cold air from entering the lungs. That there is a considerable danger of these occurrences in the case of young and delicate children, I am pretty certain; and I should be glad to learn the experiences of others on this subject.

We now have to deal with the last and most important danger—invasion of the medullary centre and stoppage of respiration. Ether there falls short of the ideal as an anæsthetic, in so far as it travels beyond merely annulling sensation, and attacks vital centres. However, considering the great volatility of ether, and that the advent of apnœa is heralded by marked signs, while the heart remains in active function even after apnœa, we have, I submit, a far less alarming symptom with which to deal than in the case of chloroform-apnœa.

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[1] Chloroform Committee, Royal Medical and Chirurgical Society. Scientific Grants Committee, British Medical Association. Anstie, *Stimulants and Narcotics*. Sansom on *Chloroform*. Bowditch and Minot (*Boston Medical and Surgical Journal*, 1874; quoted by Wood).

Among the anæsthetic agents belonging to this group, but few, save chloroform and ether, are widely known or largely employed. Methylene, or bichloride of methylene, ethidene, and amylene, have been received with some favour.

Methylene was carefully studied by Dr. Richardson, who gave it a very high commendation. His views were early called in question by Nussbaum in Germany, and Tourdes and Hept and Péan in France, while Sir Spencer Wells took up the cudgels in defence in this country. Subsequently, several deaths occurring, the belief in this agent became shaken. It was further objected by the Anæsthetic Committee of the British Medical Association that the methylene was probably not a simple body. Two years ago, MM. Regnauld et Villejean (*Journ. de Pharm. et de Chim.*, 1883) undertook a research which has recently been completed, and which led them to the following results: that the commercial methylene, obtained through agents accredited by Sir Spencer Wells, and therefore genuine, was a mechanical mixture composed of four parts of chloroform and one of methylic alcohol. Their proofs I cannot detail. They next investigated true methylene dichloride—that is, methene bichloride—a substance very difficult to prepare pure, and very costly. In some comparative experiments, they administered the two agents, finding that, while the commercial agent behaved precisely like chloroform, the genuine methene bichloride produced choreiform and epileptiform convulsions. Unless any flaw can be shown in the work of these gentlemen, methylene cannot, I take it, be accredited with any virtues greater than belong to the long known mixtures of chloroform and alcohol. Obviously, no further discussion on my part is needed.

Ethidene and amylene have, unfortunately, been discredited, as deaths have occurred during their use. Those attributed to amylene are, according to M. Péan, attributable to causes other than the agent employed; and the same may well be said of several, notably the one recorded by Mr. Clover (BRITISH MEDICAL JOURNAL, 1878), with regard to the fatalities of ethidene. The behaviour of the group before us reveals, in a striking degree, the working of laws which connect their behaviour towards the organism with the molecular weight of the agent. Thus, Dr. Richardson, comparing a number of them, says: "There appears to be reason for the belief that the lethal energy of an anæsthetic is clearly related to the molecular weight of the substance, increasing directly as its weight increases." However, in summing up the case of the various members of the carbon-series in which chlorine occurs, I think we must admit that, as far as the evidence is now before us, they are all dangerous anæsthetics. But we must remember that the danger of which I speak must, in some cases, be a necessity; and, as a practical anæsthetist myself, I should not hesitate in many conditions to administer chloroform, or a congener, rather than ether.

The thoroughgoing advocates of ether will tell you that ether can and should be administered in all cases, or, practically all cases. They would commend rectal etherisation when faucial etherisation was impracticable. The practice I deem too dangerous for general use, but we must consider it as a possibility.

Turning, however, from the broad issue as to which agent among the general anæsthetics is the safest, we must answer the question—May not a truly local medication be effectual in promoting an absolute anæsthesia?

Time will not allow me to enter upon the subject; it alone remains for me to ask you to consider how far a local ether spray, the use of rhigolene, or the employment of eucaine, will fulfil the duties of the ideal anæsthetic. I should, from my experience of the former two methods, relegate their use to the most trifling operations of minor surgery, as their powers of promoting anæsthesia are very small, very limited, and very transitory; while, at least with ether-spray, its use is attended with inconvenience to patient and operator.

Of eucaine I will say little; there are others here, far more competent than I am to expound its uses and extol its merits; I will only say that eucaine has probably achieved a position from which it will be hard to oust it. It does not appear to have answered in every case, and its range of usefulness must, of necessity, be restricted within narrow limits.

In summing up, then, I would venture to ask for a careful consideration of the physiological bearings of the question before us; let us take, step by step, the evidence and weigh it in the balance of experience; let us dispassionately judge between the substances under review; and, having arrived at some decision, let us then inquire into the best methods whereby the selected substance can be most advantageously administered. In conclusion, I would remind the Section that I have been, on this occasion, able only to compare the physiological actions of ether and chloroform, so that I must reserve to myself to resume the inquiry into the physiological behaviour of other substances used for producing anæsthesia, upon another occasion.